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## Certain Aspects of the Species Question.

BY EDWARD L. GREENE.

Some months ago, while endeavoring to reduce in some degree the confusion of an overladen and disordered work table, I found one corner of it occupied by a formidable stack of pamphlets most of which I had wished and meant to read, yet none of which I had seemed to find time to peer into. Attempting to assort these, I came at length upon one most inviting looking blue-paper-covered brochure, something about which constrained me to pause and rest awhile, and read.

Opening at random this fair document, my eyes fell on this caption: "The Taxonomic Aspect of the Species Question." I could not at first read much beyond the title itself, it seemed to me to be such a remarkable one, and so ill devised, so pleonastic; for two out of four of the terms of it appeared to me to be redundant. Inasmuch as species are in their very essence taxonomic units, one cannot touch the subject of species without immediately involving himself in the subject of taxonomy. The species idea, and taxonomy, so long as species are considered from any standpoint not wholly unscientific, seem absolutely inseparable. If I am right in this, I said to myself, then this title would have been improved had it read simply: The Species Question, for the taxonomic is unmistakably implied in that, and to have written it out was to write a needless word, and the filling in of useless words makes bad writing. These were the reflections that followed my mere glance at the first title to which I came, as I opened the pamphlet; and before reading I began looking to see what the other titles were. Going back to the beginning, I found that the very first caption was precisely the same as that second one, the one which by chance had been first to claim my attention; and as I went

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\* December 1, 1910.—Pages 245 to 267.

on, I read in regular sequence the captions following: The Physiologic Aspect of the Species Question; The Physiologic Aspect of a Species; An Ecologic View of the Species Conception; An Ecologic Aspect of the Conception of the Species; and having viewed these several titles in line, and noted that unity in diversity which they evince, I saw how the taxonomists in deference to the titles chosen by the physiologist and the ecologist, had been obliged to introduce a corresponding modifier into their own title. They had courteously waived their rights as the sole real custodians of the Species Question, and had inserted terms by which, what would have been their own simple rightful caption became a distorted pleonasm. It was done out of courtesy, and is on that ground excusable and excused.

But am I to be excused by my friends physiologic and ecologic for having seemed to deny them any right in the subject of taxonomy? I shall be, when once they have understood me. If any one of these by his researches physiologic or ecologic comes upon new facts seeming to bear on taxonomy, and he make the application, or suggest, even, its taxonomic usefulness, he is in that become, for the time, a taxonomist himself.

But that which is of unusual interest in this Report of the Symposium is an aspect of the species question not named in any of the formal papers,—indeed not named at all—but which one detects in the reading of the reports of the informal discussion which is said to have followed the presentation of the titled papers, and which appears to be as far removed as possible from the scientific; even disclosing itself in a guise which leads one to question whether it may not well be designated the industrial aspect of the species question; though that may perchance be seen, by and by, to be too lax and indefinite an expression to be employed. The most entertaining thing about this particular viewpoint is the proposal that in genera where species are thought, from whosoever point of view is not stated—to be inconveniently numerous, there shall be a kind of enforced reduction of the number, and that by what threatens to be made a merely arithmetic rule. There was even a suggestion of the probable proportion of two-thirds of the species, and this suggestion met with warm approval from at least one corner of the symposium. Such suggestion, should it be adopted as a rule to proceed by in this arbitrary reduction, would work in this way. If, for example, within the last dozen years Mr. Fernald

and I have published for the region of Gray's Manual a dozen species of *Antennaria* where all former editions of that book had but one species, eight of our new ones must be relegated to non-entity, and only four be permitted to figure as valid; this number of four, we venture to suppose will have been estimated as a large enough increase of the number of antennarias over the number of one species recognized twenty years since. Again: if I alone have published for the United States and Mexico 45 species of *Ptelea*, where formerly 5 only had been accredited, I am warned beforehand that by this arithmetic gauge of taxonomic values 30 of my *Ptelea* species will be invalidated and 15 out of 45 may stand that strange and wonderous test and be approved. The question how these reductions are to be accomplished does not appear to have been broached by any one of the two or three members of the symposium who suggested and with more or less enthusiasm advocated the arithmetic scheme. Presumably the selection will be made by lot. That would be in perfect keeping with the principle advocated; though possibly some other alternative would be resorted to, by which to show favor to such "recklessly made" species as the foremost and most willing if not ardent supporter of arithmetical suppression himself aforetime perpetrated. By way of illustrating what I mean, I shall call attention to the fact that much less than a generation ago the admitted species for the genus *Eryngium* in North America numbered 15. At present they number 30, and for this recent doubling of the number of species in the genus Mr. Coulter is mainly responsible. Two or three other authors have contributed barely, or scarcely, one new species each to this increase, while for 11 of them our main supporter of suppression by arithmetic rule is sponsor.

Yet again, in that earlier time which is not long ago, to the group of plants then called *Peucedanum*—afterwards within a little space twice changed, first to *Lomatium*, then to *Cogswellia*—there were attributed some 25 North American species; then all at once, as late as the year 1900 the membership of the genus was more than doubled, and that not only by consent of our species-fearing friend, but by his most active support; for, out of these now 60 species most of the 35 new ones are owned by him as his. And these are but examples selected out of considerable number in which our most strenuous supporter of the arithmetic reduction

scheme, will have to apply the process to his own work, provided such general suppression of recent species be taken in hand.

I have been more explicit than I had intended to be in open animadversion on this aspect of the species question; but I saw the desirability of calling forth this strange suggestion from its lurking place in the symposium report, and setting it before another class of readers for their enlightenment as to what curious things are being thought and spoken in relation to species; and no one, I think, will have the hardihood to undertake the defense of those propositions as even remotely verging toward the scientific. Probably nothing more dogmatic and arbitrary ever before found expression, and at the same time was let pass uncriticized, in a symposium of men professedly scientific.

I have already referred to this unscientific outlook on the species question as the industrial one. Possibly it may prove susceptible of a more definitive or diagnostic name. Let us see. I do not recall that the scheme of arithmetic species-reduction had advocacy in the symposium save on the part of such as are known to be individually interested in copyrights on books of descriptive botany; and this is in all probability more than a mere coincidence; any way, I have long seemed to see that there is often taken, however disguisedly, what I will designate now as the Bibliopole Aspect of the Species Question.

It is evident that if good profits are to accrue from botanical manual copyright, two conditions must be met. The book must profess to cover an area of well peopled territory sufficient to invite many purchasers. Then the cost of the printing must be so inconsiderable that the book may be offered at a moderate price. Of course the expense of publication is determined by the number of pages printed; and so one, and perhaps the most inexorable, of the conditions of a volume of descriptive botany that is to *pay generously* will be that the number of species to be described be reduced to a low figure. The smaller the number of species the less the cost of printing and the more lucrative the investment, so long as the extent of territory embraced, and thereby the prospective large sales, remain the same.

By way of illustrating how a single genus, according to the treatment given it, may increase or lessen at once the cost of printing a manual, and the profits on copyright in it, let us consider *Crataegus*. It is well known that, by recent investigation

that has been carried on by a considerable number of botanists, it has come to pass that, where the last generation of manual-makers had less than a dozen species for the whole of the United States and Canada, those of to-day are confronted by nearly a thousand that have been formally named and published within about twenty years; most of them, too, with diagnoses—not a few of them with illustrations—that in general seem to bespeak valid species, however astounding to us conservatives the fact may seem.

And now, what is the aspect of this *Crataegus* question from the book-sellers point of view? Suppose that good descriptions of 10 species, printed in the type usually selected for manuals of botany, fill two pages, the printing of which costs \$4.00, then diagnoses of 100 species will fill twenty pages and cost \$40.00, and 900 species would cost \$360.00. Now if the more than 900 North American kinds of *Crataegus* which most reputable botanical gentlemen stand sponsors for, and whose validity they are ready to defend, can by hook or by crook be reduced in number by two-thirds, then the cost of printing *Crataegus* for a North American Flora will fall from \$360.00 to \$120.00; and so, if I figure correctly, it looks as if, upon my hypothesis as to printers' bills, the neat sum of \$240.00 might be added to the profits of copyright on the score of this one genus alone, by the copyright owner who can reduce the now existing catalogue of its American species by two-thirds. And this illustration which I make by this extreme case of *Crataegus*, throws light enough upon the whole affair. If some hundred of smaller genera, such as are credited with but a tithe of 900 species, can one after another be cut down in said arithmetical proportion, the reduction of their species will of course in the same way augment the profits in copyrights.

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There have been generations of good botanists who lived peaceable and comfortable lives, also in the enjoyment of tidily remunerative copyright in manuals, all undisquieted—or at least seldom and not deeply disturbed—by the species question.

Away back in the year 1848 there was no great dearth of manuals of descriptive botany for the northern United States. Amos Eaton's Manual had gone through eight editions, and Wood's Class Book has appeared in a tenth edition. In this year there came forth a new Manual of Botany of the Northern United

States, issued by a good publishing house in Boston. The territory embraced by this first edition of Gray's Manual extended from Maine to Wisconsin, and far enough southward to include Pennsylvania and Ohio. In an exceedingly well written and instructive Preface we meet with the statement that it has been "firmly intended to restrict [the book] to 350 pages;" and here we seem to have intimation of a requirement of the publishers that the pages of this Manual must be few. They were, like publishers in general, apprehensive that too many pages would reduce the profits; and what troubled them in this instance was—had they but known the real name of it—the species question.

But this initial edition of Gray's Manual—by much the most handsome, as well as scholarly in its substance and make up of all the editions—came out in more than twice the 350 pages, and was a remunerative enterprise nevertheless. And now, in order to show how serenely, and during half a lifetime, the author of Gray's Manual escaped the troubles of the species question, a comparison of some sort must be made between the taxonomic contents of this first edition of 1848, and the sixth edition of 1889. To accomplish this it is only needful to make selection of several representative genera of higher plants, and contrast the number of species attributed to each such genus in those two different editions the dates of which indicate an interval of 41 years.

	1848	1889
Antennaria	1	1
Arabis	10	9
Cnicus	10	8
Crataegus	7	10
Eupatorium	13	16
Hieracium	6	6
Potentilla	19	14
Rubus	10	11
Solidago	32	42
Viola	17	17

These figures, as I have with care studied the genera as they appear in the two books, have proven an astonishment even to myself; for, while from my very boyhood forward to early manhood I had heard it said that the author of this manual was dogmatic, and intolerant of any questionings of his own infallibility on the species question, still, after all the years, I was not yet so

fully convinced as the above mentioned and other comparisons have convinced me, of the absolute stagnation of botany in the Eastern and Middle States during the forty years and more of this author's unbroken rule and acknowledged sovereignty.

One of the interesting points, as to the condition of botany during those long years, is that in certain genera there was even a reduction of the number of species, and this despite the circumstance that the territory covered by the manual had been notably increased. And if *Eupatorium* and *Solidago* show a slight increase, this is solely because certain southern states—the South is rich in species of this genera—had been annexed to the manual area.

Now it was not at all held as a secret by the author of this series of manuals that the copyright on them was about his best source of income. I myself have heard him say it. It also appears that in this first edition of the work, the one which he had hoped could be made a good one within the limits of 350 pages, but which grew to pages 696—in the preparing of it, I say it appears that he had been most unsparing of the published species of his most eminent predecessors and contemporaries. I doubt not he may be found to have suppressed in the aggregate some hundreds of the species of Michaux, Pursh, Nuttall, Rafinesque, Muhlenberg, Eaton, Dewey, and almost all those of his Bostonian neighbor, Jacob Bigelow, than whom in my judgment, no one then living in New England had better native abilities as a botanist. And this reckless overruling of his contemporaries—many of whom had field knowledge of northern botany to which he never attained or even aspired—this dogmatic suppression of others' works, supported by that well deserved prestige which his *Flora of North America* had gained for him, together with that influence which high professional station exerts over the whole multitude of amateurs and aspirants—all these conditions together brought about the period of botanical stagnation which is now well recognized as having existed during long years.

The dormant period of East American botany had been brought to an end some years before the appearing of the very latest edition of Gray's Manual; and as the author of the first edition sixty years before, as prospective bibliopole, had found himself confronted by the species question, so, and also under pressure of bibliopole considerations, did the editorship of the newest edition find itself face to face with the same difficulty, and that in an ex-



aggregated form, as well as under conditions much less favorable to a dogmatic and imperious solution of the problem than those which had existed sixty years before. The first editorship had been vested in a single individual, a man already of reputation, and that also well earned, in his chosen field, gifted with insight and in his laying siege to species singularly favored by circumstances. Of his elders and his contemporaries who had published so many species, almost all had disappeared from the field; some as having been claimed by death, others as having abandoned botany. He was even a professional botanist more than his predecessors, and occupied a seat of authority. Everything conspired to warrant his assuming, for North America, the imperium of the science. It required no great degree of courage to assume it; and his imperium was long a success. His task was easy. There was not at the time, a journal of botany in existence, nor any danger from open criticism or protest. Of those expensive abstractions, species, he suppressed as many as he would, and then for almost forty years, enjoyed the remunerative and the peaceable fruits of his early conquest. How, and by what forces, this imperium came to an end need not here be told. The important fact is that it had ended a full decade of years before the latest issue of Gray's Manual was undertaken; and species had multiplied amazingly beyond all that any one forty years earlier could have dreamed; and the editing of the new manual involved difficulties impossible to have been anticipated. With what indifferent results compared with those reached sixty years before, the new editorship has met the species question, may be intimated by presenting again a column of sample genera; a list of generic names in good part the same as that given above, but showing figures for the three editions of the Manual, the first, the middle edition and the latest.

	1848	1889	1908
Antennaria	1	1	11
Carduus	10	8	15
Cratægus	7	10	65
Eupatorium	13	16	19
Galium	10	13	25
Hieracium	6	9	17
Houstonia	5	6	11
Juncus	20	27	44
Lechea	3	5	8

Lonicera	8	9	15
Panicum	19	23	83
Paspalum	2	8	21
Potentilla	9	14	21
Rubus	10	11	38
Scirpus	14	17	35
Senecio	3	8	18
Sisyrinchium	1	2	13
Solidago	32	42	56
Vernonia	2	6	8
Viola	17	17	45

Perhaps a selection of figures statistical like the above does of itself sufficiently emphasize the failure I had intimated; the the manifest inability of the aforesaid editorial people to suppress the swarming multitude of recent botanical species. To suppress such by the hundred would have seemed the bounden duty of new editors in view of their having chosen—perhaps by forceful considerations having been constrained to use—the name of that great conservative who was so intolerant of species and species-makers. I am confident that the author of the first Gray's Manual could he have had in dream or vision of the night a view of the three columns of figures written above, realizing that those of the right-hand column were exactly prophetic—a real enumeration of the species that were to be admitted in a book of the not distant future and to be audaciously named “New Gray's Manual”—not only would he have arisen from such a vision in a rage; he might have lived thereafter to his life's end in dismal apprehension of a reincarnate Rafinesque, coming with power to beget speedily a numerous and full-grown progeny.

It must not be assumed by the reader that the genera cited are in all cases indicative of sudden increase by accession of so many new species. *Hieracium*, for example, had in 1848 6 species, all native; and the 3 more that find place in the book of 1889 are all Old World species that had added themselves to our flora by naturalization. Also the increase from 9 in 1889 to 17 in 1908 is mainly attributable to the same cause; for now 7 out of the 17 species are introduced, 1 only having been added to the aggregate of species as new. *Carduus*, or *Cirsium*, or by whatever name you call the genus of the thistles, as well as *Galium* and *Senecio* have of late been more or less notably increased in the number of their

species by the same circumstance. *Lechea* I have written into the list of selections partly because that genus figures as perhaps the very first to have been seriously and critically investigated, and to its increase of species, while as yet Asa Gray at least with respect to eastern botany was holding the imperium. It is also worthy of note that the amateur, Mr. Leggett, who had been so successful in his researches in *Lechea*, had hardly gathered courage to publish his results, and the publication of them was made by bolder men after the demise of the investigator. I observe also that, beyond all doubt in my mind, the species of *Lechea* in the edition of 1908 ought to have been 10 rather than 8, the editorship having reduced two of a New York botanist's species which, from their original characterization, seem as valid as any of the other five that came into their places late, and are recognized in the new book as good.

But, of entirely another category than that to which *Cirsium*, *Hieracium*, *Senecio* and *Lechea* belong are most of the genera listed above, in that the sudden and great increase of them in species which my figures indicate—a simply enormous increase in the cases of *Antennaria*, *Cratægus*, *Panicum*, *Rubus* and others—has come about solely by an accession of new species, all established within much less than twenty years; most of them within twelve or fifteen. I say established because the editors of the new book concede that all the species—in whatsoever newly expanded genus—which they have admitted, are valid species; such as they themselves have passed their supreme judgment on, and that favorably. Here are their words: "The present editors have considerably delayed the issue of this work in order to examine these new propositions and give them recognition in all cases where their merit could be demonstrated. In a few instances, however, it has been impossible from lack of material or data either to include as valid or to reduce definitely to synonymy such species and varieties, and it has accordingly seemed best not to mention them. It is not thereby meant that they are not of value, but merely that evidence of their distinctness has not been available."

Now from this pronouncement, and the explanation, which together read so open-mindedly and fairly, one need not overlook certain curious logical inevitable conclusions that follow.

There is in Massachusetts a seat of high judgment on all matters of systematic botany, a court of final appeal before which

every proposed new species for a certain large area must appear as for trial before it can be allowed to pass as valid. Though possibly good enough, unless approved by this tribunal, the faithful and discreet among botanists will not admit its validity, and must treat it as non-existent.

Of course, this kind of assumption—presumption, rather—is no new thing. There is another self-constituted tribunal of the same pretensions in New York, and this one, from certain points of view has better claims to being considered seriously. The members of its judiciary are much more experienced in everything relating to taxonomy. Only a moment ago we were taking note of the fact that New York, as reported by the symposium reporter, had in so far canvassed the species question as to be able to offer at least a rough estimate of the numerical proportion of recent species that might be suppressed; and, considering everything, that is no more startling than the statement of the New England judiciaries, that they, at whatever expenditure of time, and delay of publication, have been able to examine the data, and to decide upon the “merits” of most of “these new propositions,” and with the result that only “in a few instances,” and by lack of satisfactory data, have they failed to attain to sound conclusions as to the validity or invalidity of them. And that which to many a mind must seem extraordinary is, that in the cases of certain genera that have been most extremely enlarged by accession of new propositions, they have found the smallest number to discountenance, even often next to nothing to reduce or suppress. Their treatment of *Sisyrinchium* is apposite; for from a genus of 1 species in the edition of 1848, it had increased to one of 2 species in that of 41 years later; then in the space of hardly more than 10 years before the edition of 1908, it had acquired, in the opinion of our New Manual editors, as many as 11 more, making an aggregate of 13. For my own part, after thirty years or more of botanical travel and sojourning east and west, with frequent close inspection of members of this genus everywhere, and after the publication of a number of western and southwestern species, I should not have doubted about the possible, or probable, existence of even more than 13 species for this Gray’s Manual region. As long ago as 1868, on the prairies of central Illinois, I came to realize the perfect validity of *S. albidum*, now of late restored; and in 1869, when my correspondence with Asa Gray began, I realized the utter futility

of attempting to impress such an idea upon the mind of that botanist. But now, as to the difficulty of the task which the editorship of the New Manual had before them in the case of this genus, my friend who did nearly all the original research on eastern and middle-western materials of it, will pardon my saying that, on account of great length and fullness of his descriptions of the new species, I could never hope for leisure to go through them all, and choose out the really distinctive characteristics of them; and that an editorial tribunal of the manual, itself unpracticed in sisyrinchial problems, could accomplish all this, and with even the bibliopole aspect of the species question unforgotten, could set the seal of approval to 13 species of this genus, almost all of them new, and also could "reduce definitely to synonymy" 3 others—all this is, under all the adverse conditions, a verdict judicial that, for the moment commands my admiration.

Less wonderful to me is a similar judgment rendered in the case of *Rubus*; for, notwithstanding that the accession of new species for this New Manual region, as admitted even by our editors, amounts to about twice the whole number ever before recognized in any edition, those new specific propositions—there are more than two dozen of them—have been presented to the public under conditions more than ordinarily favorable to their ultimate approval as valid. The very best data for new types are good descriptions; they are even better than good specimens, albeit good descriptions are perhaps as rare as are the professed taxonomists even who are competent to read and understand them. But Dr. Blanchard's descriptions of new brambles, as they have appeared from time in *Rhodora*, have impressed me as most lucid and satisfactory. I have adjudged them the most complete and the most intelligible diagnoses of new types that have ever been published in New England, and have felt that by them alone I might feel assured that his species were good, and also that I should be able readily to identify them by the diagnosis.

Here also I may as well remark that despite my having long been in rather proud possession of several unsought written encomiums on my own plant descriptions, some of these from the hands of men no longer with us, but who were of unquestioned high rank in matters of phytography, I am still half willing to attribute to failure on my part as diagnostician in this instance the fact that, out of 6 clear species which I imagined I had clearly con-

tributed to the phytography of this genus, one only has the editorship been able to "reduce definitely to synonymy", the other 5 having been left wholly without mention. If it were an exceptional or isolated case of the treatment of my endeavors to improve the Manual region botany, I should freely accuse myself as an occasionally stupid phytographer; but instances of this kind of thing are so very numerous in the book that I begin to think there is another and still unnamed unscientific point of view, for which I have been considering the appropriateness of the designation, The Clansman's Aspect of the Species Question. I doubt however, about that name as sufficiently well devised. It may be too mild and lenient suitably to designate the invidiousness of that particular point of view. But the clannishness of two contending sets of American botanists has well been noted on the other side of the Atlantic; where also they themselves may be open to the same criticism. At home, I hope it may be coming to be recognized that we have one botanist, whose prominence before the entire world botanical, whose strong influence at home none will dispute except in whisperings behind his back, who openly and serenely defies the clanishness of both clans, conscious of power to stand, to dare and to do against the envy and the malice of the envious and malicious who abide among the kind and friendly in either clan.

There is another genus named in the list above on the specific growth of which I can not forego brief remark. In the new book *Panicum*—taking the genus in that broader acceptance of it exemplified in the edition of 1889—has grown from 23 species to 83. In nineteen years, then, 60 species have been added to it most of them new, and that is saying that the membership of this genus has been almost trebled within less than 20 years; and a noteworthy fact about it is, that only 15 species that have been described as new have been reduced to synonymy.

This *Panicum*—a most critical and difficult genus—and the 82 other genera of the grasses, have been prepared for the book by a contingent of the editorship which seems to have had grace to avoid party entanglements, and in its work does not evince distinct alliance with either clan. In the fulfilment of its task it does not seem to have been obliged to take for a moment the bibliopole aspect of the species question, and has been permitted to do its work in a spirit of candor and simplicity; the only spirit by

which to ensure truly scientific results. Very possibly a spirit of unalloyed sincerity has ruled the revision of the Cyperaceae; for here, for the whole family, a very large one, rich in species by American authors of the early and the middle periods of the nineteenth century, the editor has put forward but 10 new ones of his own specific propositions, and has suppressed only some 25 that have been proposed by other authors of his own time.

The statistics of *Crataegus*, as I have given them above, appear startling; for we who have had too much else to do all the while have heard all sorts of tales about superlative species making in this genus, and all within the last ten years. The figures show for the new manual, only a six-fold increase in the number of *Crataegus* species since 1889; and *Antennaria*, a genus which has been much less talked about, has increased in the number of admitted species more than tenfold. But then, of the 10 species which in the new book are added to the one which was in the older books 7 are mine; the other 3 also appeared for admission later, and as under my immediate discipleship, and but for my having shown where the characters for *antennaria* species are to be found, it is scarcely probable that the book of 1908 would have differed from its predecessors as to membership of *Antennaria* for the Manual territory. But there was with me a limit to the possibilities of *antennaria* expansion; a limit set by nature, apparently; for I believe I did not discover and publish, for the whole of North America, to exceed 35, or at most, 40 species. How very different the history of *Crataegus* within the same recent period. I have been assured by one who has had some part in the investigation of that genus that about 1000 American species have now been published; even the greatest proportion of them haling from the United States and Canada. It appears that about 600 species had been added to the genus for the Gray's Manual region at about the time of the issue of the 1908 edition. And so, inasmuch as said edition has but 55 species over and above the 10 species of the issue next preceding, the 600 proposed new ones have been brought down by the suppressive skill of the editorship to the remarkably small number of 55. Then again making a canvass of the *Crataegus* pages of the manual in quest of such of the new ones as the editorship has been able to "reduce definitely to synonymy," if no error has been made in the counting, the coincidence is curious, that these also number 55; and we have now,

as named and accounted for in one way or another, the sum of 110 of 600 published new ones, while the "few" which remain totally without mention are 490. Considering the hundreds of genera treated in the volume, and the very great number of them in which new species have been published within ten years, and noting as we do many large genera in this flora, to some of which decades and even scores of species have been contributed within a dozen years, besides a much greater of small genera which have been thus augmented in the same proportion, in the aggregate of which only here and there a newly added species has met with any mention in the book, it may be inferred almost to a certainty that a total of a thousand or more of this sort exist; and a thousand can not be called "a few." The botanical public may claim a right to more well considered and fairer statements than this and others which seem to have been thrust into this Preface without reflection, and, I dare add, without conscience. In order to show warrant for so severe a statement as this, and at the same time to impress more deeply the truthfulness of it, I go back to *Crataegus*; and in resuming the thread of former discourse upon it I shall first confess to have taken something like self-gratulatory satisfaction in following the extraordinary increase of late years in the proposed species of this genus. Me, to whom aforetime all the making of superfluous new species was attributed—me have the several specialists in *Crataegus* so far outstripped, that I am left almost an extreme conservative, in peril of coming to be called a "moss-back." What are my two score antennarias and three score violets—all of them herbs, and of plastic and mutable nature and temperament—what are these few scores of my species here, compared with 600 new species in one genus of trees, all on the same extent of territory? And it is a genus not only of trees, but of slow growth and slow and scanty multiplication of individuals; a genus in which the evolution of distinct types must proceed with incalculable slowness in comparison with such things as violets, the individuals of which may be presumed to multiply a million a year to one of *crataegus*. I suggest to my friends of the Chicago Symposium that they cease from quoting the Jordans and Gandogers of Europe for their illustrations of excessive species making. The plant they call *Draba verna* inhabits almost all temperate Eurasia and America, flowering in untold millions of new individuals every year, in multitudinous diversities of soil and climate; and 60



definable segregates of this would seem a hundred times more probable than 600 segregates of *Crataegus* in northeastern and middle western North America.

At the beginning of this development of white thorn species, I regarded the rapid progress of it, as did others, with amazement and was disposed to be altogether incredulous. The whole proposition involved too many improbabilities. I have not attempted any study of the forms existing in any locality where I have been, I have never published more than one new *Crataegus*, and that was done before the new movement took its rise, and had nothing to do with it. There are, however, some recent experiences of mine which have induced quite a change in my attitude toward the crataegus movement. In my annual excursions westward, northward and even eastward, I have met with more than one, or two, or three local amateur botanists to each of whom in his own district, a number of white thorn species was known familiarly, and by name; men who would disclaim all accurate knowledge of their numerous violets, meadow rue's and antennarias; and the reason of their familiar acquaintance with a number of crataegi I soon learned. The locality of each such resident botanist had been visited by Professor Sargent. They had conducted him to their woodland border thickets, their open slopes and their more level cow pastures where there were thorn trees, and he had indicated to them the specific marks of the different kinds; and so, such local disciples of his were able to remove many a doubt of mine. I have been brought to an attitude of receptivity on the crataegus question. But what is more germane to the argument is that, since the new manual came out, they fail to find therein so much as the names of some of the species that they had reckoned among the best. Within the two seasons last past, I have heard this same complaint from different people and belonging to localities remote from one another; nor could it be otherwise if, as it plainly appears now, 490 out of the 600 newly published kinds of thorns are left without mention in the book.

In one place and in another, quite within the manual limits I have been conducted by local botanists,—the names of several of them are of reputation in the science—to the various habitats of several of the violets of their vicinity, species whose distinctness every plant lover of the district is so well assured of as not to doubt their title to the specific names that have been given them, and had

been given them as new in the interval between two editions of Gray's Manual; and this done, I have been asked why such unquestionably good species have not been so much as mentioned in the new manual. This experience has been mine in two or three different and widely separated states and provinces that lie within the limits which the book pretends to cover.

Over and above the multitude of the teachers and their pupils in the schools, there are two classes of what we may call lay botanists collectors and amateurs, into whose hands the manuals of botany fall. These outsiders, if we may so designate them, are of service to the science in many and in different ways; and they are of two distinct classes. One class studies nature and observes and compares plants, each man of them with his own eyes, and forms some opinions of his own and does not renounce these opinions for the mere reason that several books of descriptive botany set forth opposite views.

The other class distinguishes itself by looking at nature and studying plants, as it were only through the eyes of the men who have written books. These will seem to hold in such deep veneration the newest edition of their chosen manual, that they will force Dame Nature herself to conform to the rule and dictum of the authorship of that one book. The volume will seem to be regarded as scientifically without spot or blemish, and an absolute finality on all questions of the rank of groups, and of the nomenclature of them; and if a man of this type should ever engage in a bit of real research that would seem to threaten the soundness of a dictum of his editorial scientific lords and masters, he would be likely to drop the investigation at the bidding of such authority, lay his fingers to his lips and whisper *peccavi*.

Each of these types of the lay botanist does useful, even admirable work for the science. They explore, singly or collectively the native vegetation of a town, a county, or a state; they gather personal or club or society herbaria, and these are first-class repositories of fact, small matter what little book is sworn by in matters of classification and naming, whether Robinson and Fernald's—so very ineptly named "Gray's"—or Britton and Brown.

And the independent class, they who dare to have ideas of their own, and venture to look into nature freely, and fear not the botanical dogmatist—these, while their attitude, and theirs alone is the really scientific attitude, and the one sure to be the most

fruitful of results,—these are apt to display a forwardness in proclaiming their discoveries which the venerative and submissive party is saved from.

If freedom of botanical research, and the practice of scanning nature with one's own eyes rather than through those of dogmatic, domineering and repressive makers of books,—if this, on the whole, and scientifically viewed, is the better way, then that was a propitious day for East North American botany which gave to it Britton's Manual. Even though in respect to this book there would be found a contingent who would pay it undue veneration, this was of no moment compared with the fact that the right of the old imperium to its usurped supremacy should be in this bold emphatic manner disputed.

This new manual, emanating from New York, affirmed,—and eventually, along with other great botanical events, each of equal import, confirmed—the strong establishment of a new centre of zealous work, and the seat of a powerful rival authority on systematic botany. The book itself looked almost thin and small by the side of the Gray's Manual of 1889; but that was because of differences in paper, and in the compactness of the pages in the New York volume; but when this book was opened you found it contained almost 1100 pages of this small-type printing in crowded paragraphs. The Gray's Manual of the edition next preceding this, with its more comely setting and paragraphing, giving to less matter much more space, showed but 750 pages. Possibly,—I may even venture to say probably—had the matter of the Gray's book of 1889 been printed in the same style and type, and on the same thin paper, the whole would have been reduced to 600 pages if not to 550, or half the bulk of the Britton book. I feel quite sure in saying that the matter, mainly descriptive, of Britton's Manual of 1901 must amount to one-third more than that of the edition of Gray which was practically its contemporary book. And the very much greater bulk of the New York book will be found partly due then to hundreds of excellent species of early authors which, suppressed without reason in all the editions of Gray, Dr. Britton and his collaborators had investigated, found valid and restored; also partly, and more largely to new discoveries which had been made in an empire of botanical ground which Dr. Gray all his long life time had supposed to have been long exhausted.

And now, there was I think little or no working here from the bibliopole standpoint on the species question. It appears as if there must have been an honest purpose to describe all the known vegetation of the territory embraced, regardless of the cost of printing, and in trust that the work would nevertheless be a success financially; and in this there was no disappointment if one may judge from the number of new impressions that have been made within the 9 years since the first issue was given forth.

I am unwilling to put a period to the expression of these thoughts without adverting to another manual; one which, as to the great cost of publication, and the author's apparent indifference to the risk of pecuniary loss or gain, ought to be distinguished and celebrated as in contrast to the penuriousness that governs manual making under corporate institutions that roll in wealth. Need I name, after such preamble, the book and its author? Certainly I mean Dr. John K. Small's *Flora of the Southern States*. Printed in good style, on good paper, it is a volume of almost 1400 pages of purely descriptive botany; and I can not doubt that in the printing it has been the most costly—even by far the most expensive—one volume of such work that has been issued in this country; and at the low price at which the book has been sold, I do not see how the author could have expected to make profit. It may well be that the principal invested has by this time been restored; certainly very little of the interest on it, if any at all. It is a great piece of good fortune to the botany of that subempire, our South, that an able and zealous botanist can afford to do his chosen work, and issue his splendid volume without descending to take the unscientific and bibliopole aspect of the species question, and can not go down to that abject viewpoint, the clansman's outlook on the same.

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## A New Genus of Rubiaceae.

J. A. NIEWLAND.

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The genus *Galium* of Linnaeus is composed of several distinct types of plants, as, for that matter, are most of the Linnaean genera. It is a composite of several older ones, especially *Aparine* and *Galium* proper, both names originating with the ancient